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ESP EXPERIMENTS WITH SIMULTANEOUS
ELECTRO-ENCEPHALOGRAPHIC RECORDINGS

BY S. C. WALLWORK

THE intention in performing these experiments was to discover whether it is possible to distinguish between correct and incorrect ESP calls by changes in the electrical activity of the brain of the percipient. If such a distinction could be made, it would not only give some insight into the ESP process, but it might enable a weak ESP faculty to be detected without the necessity for carrying out a large number of experiments for the application of statistical methods. It is reasonable to suppose that a call which is correct by chance would be accompanied by a similar type of brain activity to that of an incorrect call, but one which is correct by genuine ESP faculty might be accompanied by a different type of activity. If these two types of brain activity could be unambiguously distinguished, then every single ESP hit could be detected.

As soon as a few preliminary runs had been completed, it was realized that it would be very unlikely that any unambiguous difference could be recognized on the EEG record between ESP hits and misses. These runs were, nevertheless, very instructive and served as a guide to further experiments, so the procedure will be described briefly.

The percipient generally wore four pairs of electrodes, two in fronto-parietal and two in temporo-occipital positions, and the potentials developed across each pair were recorded on separate channels on the EEG. The tests were carried out under GESP conditions, using a home-made pack of twenty-five cards containing five of each of the symbols O, L, S, V, Z. As each card was exposed by the agent, he tapped with a pencil directly on to the EEG record in a position close to the recording pens, thereby making a mark which synchronised the card exposure with the record of brain activity. The tap could be heard by the per-

percipient in an adjacent room through an open communicating hatch, but percipient and agent were not visible to each other. On hearing the tap, the percipient made his guess at the target card.

Three methods of recording the guesses were tried. In the first, the percipient wrote them down himself while in a semi-reclining position and with his eyes open. This method was rejected because regular, large-potential jumps, particularly in the temporo-occipital channels, indicated that movement of the eyes after the recording of each guess was causing interfering muscle potentials. In the second method, the percipient indicated his guess by depressing one of five keys on a key-board, so causing the illumination of the corresponding symbol in a box watched by the experimenter. Unfortunately, electrical interference from the keyboard completely obliterated the EEG record, and this method was abandoned also, though the interference might have been overcome by screening the keyboard. The third method, which was adopted for all subsequent experiments, was simply that the percipient spoke his guesses softly from a reclining position with eyes closed and with the minimum of muscle movement, while the experimenter recorded the guesses. These softly-spoken calls could not be heard by the agent in the adjacent room because of the background noise made by the mechanism of the EEG.

It was soon found that there was no obvious change in the EEG pattern at any point in a GESP run, and it could no longer be expected that any prominent feature could be associated with either a correct or an incorrect ESP call. It should be mentioned here that the percipient had shown significant results in previous GESP tests. Table 2 (*a*) shows the results of all the tests carried out under GESP conditions with the same agent during the first three months of experiments with this percipient. From the ESP point of view, the experimental methods and conditions were the same as for the EEG experiments, except that the agent and percipient were seated at separate tables in the same room in such a way that the percipient could not see the agent and the agent could only see the back of the percipient. Also, in some of the experiments, ESP instead of letter cards were used, but in all cases they were in 'closed packs' of twenty-five cards. After this significant series of tests, there had been an interval of about two months before any further GESP experiments had been carried out, and when they were resumed no further significant scores were obtained. The EEG experiments were therefore commenced at a time when good scores could not be expected. Nevertheless, it was reasonable to suppose that the percipient

would still be capable of making at least an occasional correct guess by ESP.

It was then considered that a more sensitive test for extra-sensory communication between agent and percipient might be the recognition of similar trends in the EEG records made from both persons at the same time. Such similarities might be detectable even if differentiating features could not be found in the record from the percipient alone. This possibility was tested by taking simultaneous records from both percipient and agent, each wearing two pairs of electrodes in posterior positions and feeding into the four channels of the one machine. No obvious similarity between the two sets of records could be found at any point. The conditions were not good, however, because the agent had his eyes open and both he and the percipient were in a sitting position.

After these preliminary experiments, a different procedure was adopted, for the suggestion of which the author is indebted to Dr J. R. Smythies. The electrode arrangement and the method of conducting these experiments were the same as in those already described in which the percipient and agent were in adjacent rooms. The agent recorded the order of the target cards on a score sheet which was kept separate from the experimenter's list of the percipient's calls until the experiment was over. Before comparing the calls with the target cards, the EEG record was examined for the extent of alpha-rhythm. Each period during which a card had been exposed was rated as 'strong alpha', 'average alpha', or 'weak alpha', on the basis of the previous records made by the percipient. A more quantitative rating could not be carried out because a wave-analyser was not available, but this qualitative classification was made by Mr E. G. Williams, the electro-encephalograph technician, assisted by the agent and the experimenter. Only after this examination was complete were the guesses and target cards compared and the positions of correct and incorrect calls correlated with the EEG.

This correlation was carried out as follows. Of the total number of periods during which cards had been exposed, the fractions having each of the three ratings were found. These fractions represent the probabilities of any one guess being associated with 'strong alpha' or 'average alpha' or 'weak alpha' activity. Also, the number of hits and the number of misses associated with each type of activity were found. By comparing these with the total numbers of hits and misses, they are converted into the proportion of hits with each rating and the proportion of misses with each rating.

If there were no correlation between success and alpha-rhythm

activity, these two proportions should be the same for each type of rating, or the difference between the proportions should be zero. This difference is not usually zero, and to test whether it is significant it is compared with the standard error of the difference, calculated from the formula $\sqrt{pq(1/n_1 + 1/n_2)}$. Here, p is the probability of a guess being associated with the particular type of activity under consideration (found as described above), q is the probability of its not being so associated and is therefore equal to $(1-p)$, n_1 is the total number of hits, and n_2 the total number of misses. The ratio of the difference between the proportions to the standard error of the difference is the critical ratio which is the measure of the significance of the difference. To be indicative of a definite tendency for hits to be associated with one particular type of alpha-rhythm activity, the critical ratio must be at least three. This analysis has been carried out for +1 (precognitive) and -1 (postcognitive) hits as well as for 0 hits on the actual target card, and the results are shown in Table 1.

From the small values of the critical ratios it is clear that no correlation has been found between the distribution of hits and the variation of alpha-rhythm activity in the brain of the percipient. Unfortunately, the analysis of scores shows that these experiments are also insignificant from the ESP point of view. This is probably due largely to the general decline in scores shown by the percipient, but it may also be due partly to bad ESP conditions caused by the discomfort of wearing the electrodes. A comparison of the scores in this series of experiments with the GESP scores in the previous significant series of experiments with the same agent is shown in Table 2.

Because of the insignificance of the ESP scores in these experiments with the EEG, it is impossible at the moment to say whether there is any correlation between the correctness of an ESP call and the nature or extent of electrical activity of the brain of the percipient at the time when the call is made. It is hoped, therefore, that it will be possible to repeat these experiments with other percipients.

The author would like to thank Mr J. Parsons for being a very co-operative percipient under rather trying experimental conditions and Mr R. B. Joynson, Lecturer in Psychology at Nottingham University, for advice and assistance. Thanks are also due to Dr D. Macmillan, Physician Superintendent of Mapperley Hospital, Nottingham, for permission to use the electro-encephalograph, and to Mr E. G. Williams for assisting in the experiments and in the evaluation of the results.

TABLE 1
CORRELATION OF ALPHA-RHYTHM ACTIVITY WITH DISTRIBUTION
OF HITS

(a) Number of guessing periods having each of the three alpha-rhythm ratings

Alpha-rhythm rating	First expt., 28-8-51			Second expt., 4-9-51		
	-1	0	+1	-1	0	+1
Strong - - -	46	48	47	48	50	45
Average - - -	94	98	92	99	102	101
Weak - - -	38	40	39	45	48	46

(b) Analysis of correlation of hits and misses with the three alpha-rhythm ratings

	Rating	Number of hits with each rating			Number of misses with each rating			Difference between proportions of hits and misses with each rating			Critical ratios		
		-1	0	+1	-1	0	+1	-1	0	+1	-1	0	+1
First experiment, 28-8-51	Strong	11	10	12	35	38	35	0.030	0.080	0.080	0.38	0.19	0.70
	Average	20	19	16	74	79	76	0.019	0.016	0.136	0.21	0.17	1.51
	Weak	8	8	11	30	32	28	0.011	0.001	0.080	0.15	0.01	1.07
Second experiment, 4-9-51	Strong	7	9	8	41	41	37	0.046	0.052	0.009	0.55	0.70	0.11
	Average	19	26	17	80	76	84	0.073	0.121	0.012	0.76	1.41	0.13
	Weak	7	8	8	38	40	38	0.027	0.069	0.003	0.33	0.95	0.04

TABLE 2. SUMMARY OF ESP SCORES

Percept—J. Parsons.

Agent—S. C. Wallwork.

(a) Previous significant series

Date	Total Calls	-1 hits	0 hits	+1 hits	Target Cards
26-1-51	175	36	51	28	ESP
3-2-51	125	26	24	27	ESP
1-3-51	275	52	70	50	Letters
8-3-51	200	31	44	48	Letters
	775	145	189	153	
	Chance expectation	148.8	155	148.8	
	Deviation	-3.8	+34	+4.2	
	Critical ratio	0.35	3.05	0.39	

(b) Preliminary experiments with the EEG

1-6-51	200	31	33	41	Letters
17-7-51	100	25	24	21	Letters
	300	56	57	62	
	Chance expectation	57.6	60	57.6	
	Deviation	+1.6	-3	+4.4	
	Critical ratio	0.24	0.43	0.65	

(c) EEG experiments with alpha-rhythm rating

28-8-51	186	39	37	39	Letters
4-9-51	200	33	43	33	Letters
	386	72	80	72	
	Chance expectation	74	77.2	74	
	Deviation	-2	+2.8	-2	
	Critical ratio	0.26	0.36	0.26	

EXTRASENSORY PERCEPTION AND PSYCHOKINESIS

AN EXPLANATION IN TERMS OF INTUITIVIST EPISTEMO- LOGY AND PERSONALIST METAPHYSICS

BY N. LOSSKY

PROFESSOR C. D. BROAD, in his article 'The Relevance of Psychical Research to Philosophy', writes as follows: 'To sum up about the implications of the various kinds of paranormal cognition. I have the impression that we should do well to consider much more seriously than we have hitherto been inclined to do the type of theory which Bergson put forward in connection with *normal* memory and sense perception. The suggestion is that the function of the brain and nervous system and sense organs is in the main *eliminative* and not productive.'¹

While fully agreeing with Professor Broad, I should like to put forward some suggestions as to the kind of philosophical system which would render explicable the facts of extrasensory perception established beyond all doubt by modern research. It seems to me that these facts can be best explained on the basis of metaphysical personalism combined with an intuitive theory of knowledge, i.e. with the doctrine that we directly perceive not only our own mental states, but objects of the external world as well.²

Personalism is the theory that the world consists of actual and potential individual centres of consciousness. A well-known instance of personalism is the philosophy of Leibniz, according to whom even atoms—or, we should now say, electrons, protons, etc.—are monads or potential personalities. These elementary entities not only create material processes of attraction and repulsion, but have inner experiences, unconscious psychoid processes which differ from psychic processes solely by their extreme simplicity. Leibniz calls such entities 'sleeping monads'. Under the influence of experience they develop and combine with one another, forming atoms, molecules, unicellular and multicellular organisms. Their life thus becomes more and more complex, and at last they become actual personalities.

Every self is a supertemporal and superspatial entity, but it imparts to its feelings, strivings, and actions the form of time

¹ *Philosophy*, October 1949, p. 306.

² A short exposition of such a philosophical system is given in my *History of Russian Philosophy* (New York, International Universities Press, 1951), an English edition of which is to be published by Allen & Unwin early in 1953.

(psychical or psychoid processes) or of both space and time (e.g. material processes of attraction, repulsion, movement, etc.).¹

A supertemporal entity manifesting itself in time is called in philosophy a substance. It is preferable to use the term 'substantial agent', in order to emphasize its active character.

Leibniz maintained that monads 'have neither doors nor windows', and that the being of each is entirely separate from that of others. It is a mistake, however, to conceive of the world as broken up into disconnected units. Every agent's manifestations are in accordance with the principles and mathematical ideas that determine the structure of time and space and are identical for all. As bearers of identical formal principles of the world's structure, substantial agents or monads are *consubstantial* with one another. The founder of neo-Platonism, Plotinus, used an excellent metaphor to illustrate this consubstantiality: he said that the human race consists of a number of men who look in different directions but are joined together at the backs of their heads. The consubstantiality is of course only partial, but it constitutes such an intimate bond between substantial agents that each one's experiences exist not for it alone, but, if only unconsciously, for all the others, i.e. from the personalist standpoint, for the world as a whole. This is of essential importance to epistemology. If I direct intentional acts of awareness, attention, and discrimination upon an external object, it is cognized by me as it is in itself, and not by means of subjective images, symbols, etc. Such direct contemplation of objects as they actually are may be called intuition.

It will be said that if direct perception of external objects be a fact, there can be no need for us to have eyes, ears, and other sense organs. According to the theory worked out by Galileo, Hobbes, and Descartes, stimulation of the sense organs is the cause which gives rise in our minds to the subjective image of an external object. Bergson rejected this *causal* theory of perception: he maintained that physiological processes in the sense organs and the cortex do not create the perception, but are merely a stimulus inciting us to direct attention to the actual external object that has impinged upon our body and may be harmful or useful to us. Unfortunately Bergson's intuitivism did not go far enough: he did not regard all cognitive acts as intuitive. Scientific knowledge expressible in rational concepts was for him, as for Kant, a subjective construct of our reason and not contemplation of reality. He thought that in addition to the activity of

¹ See my article 'The Absolute Criterion of Truth' in the *Review of Metaphysics*, June 1949.

reason we also have a faculty of intuition as the contemplation of creative living reality not expressible in rational notions.

For fifty years I have been developing an integral theory of intuitivism, i.e. the theory that all cognitive acts are different kinds of intuition.¹ My book *Sensuous, Intellectual and Mystical Intuition* is a detailed exposition of the view that sensory qualities of objects are not our subjective sensations, but properties of material processes in the external world; rational ideas—e.g. mathematical truths—are objects of intellectual intuition by means of which we contemplate the ideal aspect of the world—the aspect owing to which the world is a system; mystical intuition is the source of religious experience. A theory of intuition based upon the conception of the intimate bond between every self and the world as a whole may be called a *co-ordinational theory of perception*. On that theory the most ordinary sense perception, e.g. seeing a tree within ten yards of me, is a kind of clairvoyance: the excitation of the eye and of the visual centres in the cortex is merely a stimulus for my self to perform an act of clairvoyance in space. Bergson says that in the course of evolution sense organs have been developed for purposes of self-preservation, signalling the appearance of an object which may be useful or harmful to us and inciting the self to direct its attention upon it.

According to this view, every agent's characteristics and experiences exist not only for it but, unconsciously, for all other entities in the world. Hence it follows that stimulation of sense organs is not a necessary condition of perception. It is useful for the satisfaction of our ordinary daily needs, but in certain important cases the perception of objects remote from us in space may take place without the stimulation of the sense organs. Death, illness, or danger threatening those dear to us affect us unconsciously even though we be far apart, and, without any excitation of our sense organs, may serve as a stimulus for us to direct upon those events acts of awareness, attention, and discrimination.

Many people possess the faculty of bringing into their field of consciousness, without the help of sense organs, events of lesser moment that are of practical use to them. For instance, when they go to bed they decide to wake up at a certain hour, and actually do so; probably this is due, in many cases, to an extra-sensory perception of the hands of the clock. Professor Rhine's experiments have proved that even objects of no practical import-

¹ See my *Intuitive Basis of Knowledge* (English translation published by Macmillan in 1919), *Sensuous, Intellectual and Mystical Intuition* (English translation published by the Russian University of Prague, 1934-8), and other books and articles.

ance may be extrasensorily perceived if attention be concentrated upon them. Both extrasensory perception and precognition are explicable on the assumption of the unconscious bond between the human self and the world as a whole. They become intelligible if we give up the causal theory of perception and adopt the co-ordinational, according to which even normal sense perception is a kind of clairvoyance.

Precognition does not imply that time is unreal and that there is no clear distinction between past, present, and future. We contemplate past, present, and future as such because our self is a *supertemporal* entity and therefore can direct its acts of intuition upon any section of time.

The co-ordinational theory of knowledge is incompatible with the positivistic view of causality, according to which the cause of an event is the totality of events upon which it follows with necessity. The champions of this view have lost the true conception of causality, for they do away with the *dynamic* aspect of it and retain only the temporal sequence, putting main emphasis upon its regularity. A 'dynamic' theory of causality is more in keeping with the facts of experience; it is concerned with the *creation* of an event, and regards temporal sequence as a derivative aspect of the creative act. Events do not arise in time of their own accord, but are produced by someone. Having a temporal form, they fall away every instant into the domain of the past and are therefore incapable of creating the future; only a supertemporal substantial agent can be the bearer of creative power manifesting itself in time. Of course, a substantial agent creates a new event on the basis of his former and his present experiences. Hence it follows that we must distinguish between *cause* in the strict sense of the term, and *occasion*: the cause, i.e. the creator of an event, is always some substantial agent, and the 'occasion' are the circumstances in connexion with which an agent manifests its creative power. Uniformity and necessity do not form part of the idea of causality: no one has ever proved that an agent is compelled with absolute necessity to repeat the same actions in similar circumstances. The possibility of science does not in the least require uniformity of causation: it is sufficient that events should arise with a certain amount of regularity. In the lower kingdoms of nature, studied by physics and inorganic chemistry, this regularity reaches a considerable degree of uniformity and can be expressed by *statistical laws*.

On this view of causality there is no need for us to account for precognition by the strange assumption that the future causally affects the present: the future simply provides the occasion for us to direct an act of perception upon it. The co-ordinational

theory of knowledge leads to the conclusion that extrasensory perception in all its varieties does not essentially differ from normal sense perception, because in normal perception the activity of the sense organs is only of secondary importance.

Let us now consider the problem of psychokinesis, and, to begin with, ask the general question as to whether psychical processes can influence material processes. Take the simplest form of material process, e.g. repulsion between two entities. Repulsion is only possible as push and counter-push made by two agents, say, electrons A and B, in such a way that these two actions arise absolutely *simultaneously*, have the *same* force, and are carried out along the *same* line in opposite directions. Such simultaneous mutual determination of two objects is placed by Kant in his table of categories next to causality as a special category of reciprocity (*Wechselwirkung*). How are we to understand the marvellous correspondence of the mutually opposed actions of the two electrons? In order to explain it, we must distinguish between the psychoid or psychical *striving* to make a particular act of repulsion in a definite direction, and the actual *realization* of this striving as a material process in space. In the case of mutual repulsion of two agents A and B the initiative may belong to A, in the form of a striving to repulse B; in virtue of the agents' consubstantiality this striving is unconsciously experienced by B. If B reacts to it by a corresponding counter-striving, the two agents may realize in space a material process of mutual repulsion with equal force along the same line in opposite directions. Thus every mechanical process is psychoidly-mechanical or, at a higher stage of development, psychically-mechanical.

The dependence of material processes upon psychical in no way conflicts with the law of the conservation of energy. The only admission to be made is that the increase, say, of repulsion in one direction, required for a particular purpose, is accompanied by a decrease of repulsion in other directions so that the quantity of energy remains the same. To use Hartmann's terminology—without having recourse to his conception of non-central forces—it can be said that the influence of the psychical factor results in the transference of energy from one co-ordinate of space to another, where it is needed for our purpose. Besides, if the manifestations of an agent's energies depend upon its own psychic or psychoid states, it can be easily imagined that they may temporarily cease altogether, and the corresponding energy may pass from actual to a potential state, or *vice versa*. This explains the slowing down and the re-starting of material processes discussed by Driesch.

Nor does the doctrine here expounded conflict with the law of inertia. According to this law a body can only change its state of rest or motion under the influence of some force external to it. If an animate body could begin or cease to move, or change the direction of its movement solely through wishing to do so, the law of inertia would be violated. Suppose that on hearing some noise in the street I wanted to discover the cause of it, got up from my chair, and walked to the window. If this change in the position of my body were wholly determined from within, by my wish alone, this would certainly be contrary to the law of inertia. In truth, however, we must distinguish between an inner striving to make a movement—a psychical or a psychoid process—and the external realization of this striving, i.e. the material process of movement. When I want to rise from my chair and go to the window, my striving to push my foot away from the floor is intensified, but if the floor did not offer a corresponding resistance, my movement would not take place.

The law of entropy, on this view, must be limited, at any rate with reference to vital processes in plant and animal organisms. To see the reason for this, consider the action of the human self upon its body. By 'body' I mean here the totality of comparatively undeveloped agents allied with a highly developed agent, the human self, and serving its purposes as its organs. The human self embraces the life of its organism in one integral act of experience, and is therefore able to co-ordinate the activity of the different organs so that they work for one complex purpose and for the good of the organism as a whole. Each cell of the organism, sympathetically participating in the life of the human self, carries out the task assigned to it by the co-ordinative striving of that self. Consequently the organism behaves purposively as a whole, although many organs participate in its activities. Without using any energy the human self transforms chaotic movements into orderly ones, giving them, for instance, the same direction; hence, vital processes often are of ectropic character. In the words of Driesch, our self really is 'Maxwell's demon'.

Since a man's will is capable of influencing substantial agents which form part of his body, it is possible that in certain cases it also acts upon agents external to the body. They, too, are consubstantial with the human self and therefore unconsciously experience our strivings directed upon them; and sometimes they may comply with these strivings, or may react to them negatively. In order to prove experimentally the influence of human will upon the external world, we must direct our efforts of will upon potential or actual personalities. Such experiments may be carried out

with unicellular organisms ; this was done, for instance, by Mr Nigel Richmond in his experiments with paramecia.¹

The above considerations show that the metaphysics of personalism combined with an intuitive theory of knowledge enable us to regard paranormal phenomena as essentially akin to normal : both extrasensory and sensory perception are species of direct contemplation of objects in the external world ; the action of our will upon objects external to our body is explained in the same way as its normal action upon our own body.

A totally different character must be ascribed to attempts of influencing by will not personalities but material things, such as dice. From the point of view of personalism a die consists of potential personalities—atoms and molecules. But their combination in the die is only an aggregate and not an organic whole headed by a potential personality which could be influenced by our will. Accordingly, in this case we should have to exercise command over millions of agents constituting the die ; and it is extremely improbable that these millions should all together obey our will. If Professor Rhine's experiments prove that human will can affect the fall of the dice, this fact would have to be explained in a different way than the influence of human will upon paramecia. The following hypothesis might provide an explanation : the effort of will directed upon the falling die may be accompanied by some sort of radiation proceeding from the body and acting upon the die just as the human hand would act in pushing it. In that case a paranormal event would be analogous to the normal.

J. G. PIDDINGTON AND HIS WORK ON THE 'CROSS-CORRESPONDENCE' SCRIPTS

BY W. H. SALTER

By the death in April of J. G. Piddington one of the last links with the founders of the Society has been broken. J. G. Smith, as he then was, joined the Society in 1890, and became a member of Council in 1899. About this time he adopted his mother's surname of Piddington, to avoid confusion with several other Smiths

¹ 'Two Series of PK Tests on Paramecia', *Journal of the Society for Psychical Research*, March–April 1952, pp. 577–88. I had thought that experiments with potential personalities like electrons were not possible, but I am informed by the Editor of the *Journal of the S.P.R.* that these could be done with an electronic computer or possibly a geiger-counter.

then prominent in the Society. In the same year on the proposal of Frederic Myers he became Hon. Secretary, acting jointly with Myers until the latter's death in 1901 and resigning in 1907. He was during his term of office closely concerned in two events of importance to the Society; first the creation in 1901 of the Research Endowment Fund, of which he was for many years the active Trustee; and secondly the separation, after Richard Hodgson's death in 1905, of the American Branch of the S.P.R. and its transformation into the present American Society. Piddington visited the United States and negotiated on behalf of the S.P.R. the financial and other arrangements consequent on the new state of affairs.

From 1917 to 1921 he was Hon. Treasurer. A successful business man himself, he looked after the Society's finances with especial care, nursing the Research Endowment Fund from very small beginnings, so that in due time it might produce an income sufficient to pay a salary for a full-time research officer with qualifications and status equivalent to those of a university research worker in any of the recognized branches of Science. The income of the Fund grew indeed, but not, since the First World War, as fast as the value of money declined, so that fifty years after the Fund was started the Society still lacks an endowment sufficient to enable it to make the most of present possibilities of research.

For some years before the First War Piddington had been living at Woking as a near neighbour of G. W. Balfour, and by 1920 he had come to reside at Balfour's house, Fisher's Hill, where both of them lived until after the outbreak of the Second War. Mrs Sidgwick also made her home there about the same time, remaining there until her death in 1936. So it came about that for the period between the end of the First War and the Society's Jubilee in 1932, three leading members of the Council, Mrs Sidgwick, G. W. Balfour, and Piddington were in close contact with each other, living for most of that period in the same house, and were thus well placed to give the Society the strong central direction which in those critical times it needed.

Critical they were because the three principal founders of the Society, Sidgwick, Myers, and Gurney, were all long since dead and had become mere names to a large portion of the membership. Hodgson and Podmore, who had from early days been closely associated with them, had died before the War, and during the War the breakdown in Alice Johnson's health had compelled her to resign the post of Research Officer. The War had brought a large influx of new members, many of whom knew nothing of the past history of the Society or its standards of investigation, ap-

proached some of our problems emotionally and uncritically, and were easily beglamoured by reports of astounding successes obtained in mushroom institutions with grandiloquent titles.

How easily in such conditions spreads the complaint,

‘All old, nothing new?

Only the usual talking through the mouth,
Or writing by the hand? I own, I thought
This would develop, grow demonstrable,
Make doubt absurd, give figures we might see,
Flowers we might touch . . .
The Pennsylvanians gained such. . . .’

How seductive, even to persons with less excuse than Mr Sludge, the example of ‘Pennsylvania’, or its twentieth-century counterparts! Attempts were made, and not without influential backing, to force the S.P.R. on to a ‘positive’ doctrinal basis, to substitute showmanship for research, and, failing all else, to split the Society, but they all broke on the rock of Fisher’s Hill.

There might be a risk in any society that so close a combination of Elder Statesmen would confine the society’s activities to those in which the Elder Statesmen had a special interest. Nothing of that sort happened with the Fisher’s Hill group. Their own interest was at this time centred on the ‘mental’ phenomena—trance-mediumship of Mrs Leonard’s type, automatic writing, ‘phantasms’, experimental telepathy—and much research on those lines was being carried out during the period. But a glance at our *Proceedings* or *Journal* will show that ‘physical’ mediumship was at the same time being investigated with as much vigour as at any period of the Society’s history.

Piddington was President for the years 1924–5, and retired from the Council in 1932 after more than thirty years’ membership of it. He continued to be an active member of the Committee of Reference until 1940.

Valuable as were his administrative services to the Society, it is his part in interpreting the scripts of the ‘S.P.R. group of automatists’ that calls for special notice. This was the task of a team that included Mrs Sidgwick, Oliver Lodge, Alice Johnson, and G. W. Balfour, but the main work fell on Piddington, and increasingly so as ill-health or advancing years compelled the withdrawal of the others. This is not the time or the place to attempt a final appraisal of the significance of the scripts, but it may be a convenient occasion to remind members of the general character of a piece of work on a larger scale perhaps than any other undertaken by our Society, and of a kind that has not been and could not have been undertaken by any other body.

The problem that faced the interpreting group may be shortly stated thus. For about thirty years from 1901 onwards a round dozen of automatists, many but not all of them members of the Society, produced well over three thousand 'scripts', a word given for convenience an extended meaning to include not only pieces of automatic or inspirational writing, but also records of trance utterance, inspirational speech, and impressions received in sleep or waking or various states between. At an early stage it was noted that there were connexions between the scripts of different automatists, and also between them and the records of sittings with Mrs Piper, that appeared to be neither fortuitous nor due to normal association between them. These were the simpler 'cross-correspondences'. The connexions were usually made through the recurrence of the same phrase, quotation from the same literary source, or insistence on the same topic.

As the result of wider and closer study it was noted that the cross-correspondences interlocked with each other in such a way as to make a pattern covering a very large portion of the script material. As integral parts of the pattern were found (1) references to verifiable facts which were certainly not within the conscious knowledge of the automatist at the time they were made and had, so far as could be ascertained, never been within his or her normal knowledge, and (2) predictions relating both to public and private matters. Was it possible, by any rational and consistent method of interpretation, to establish what the pattern was, how it came into existence, and whether it conveyed any particular meaning?

This would have been a stiff enough job if the interpreters had been set to work on a final, complete set of scripts, in neatly typed fair copies, with all the quotations traced to their sources in half-a-dozen literatures, all the personal allusions annotated, and everything indexed. But in fact all the searching out of literary sources and all the annotating and indexing had to be done while the scripts were pouring out, and the interpreters had at the same time to frame, with such assistance as they could derive from directions in the scripts, canons of interpretation for a mass of material, disconnected, allusive, and symbolic. Further study made it almost certain that the obscurity was often deliberate and designed to prevent either automatist or interpreter from guessing the drift before what the script-intelligence, to use a non-committal term, considered the appropriate moment. In the Introduction to his paper in Volume XXXIII of *Proceedings* (pp. 439-60) Piddington gave a brief and lucid account of the principles on which he and his fellow-interpreters had worked. The work had been mainly done by him and Balfour in consultation, but no progress could

have been made in it without the index, the compilation of which fell almost entirely upon him.

It was a task calling for infinite patience, tireless industry, and scrupulous accuracy in detail, in all of which Piddington was highly gifted. If there was the smallest variation in the form in which a quotation occurred in different scripts, any slight misspelling of a name or error as to a date, Piddington was down on it at once. The slip might, so far as the automatist's conscious mind was concerned, be unintentional, but possibly it might be a device of the script-intelligence, as I have called it, to attract special attention to the passage where it occurred. Above all, having fixed his rules of interpretation he was prepared to follow them to their logical conclusions, even if it meant attributing to the script-intelligence intentions of a surprising kind and, as regards details of the pattern, meanings repugnant to his own robust common-sense. For it must be emphasized that he was not at all a cranky or eccentric person. Apart from psychical research, he conformed very closely in manner, opinions, and interests to the typical Englishman of his age and education. Fortunately, perhaps, although he was well-informed and well-read on many subjects, he had no special leaning towards poetry, so that in tracking down the sources of the many poetic quotations with which the scripts overflow, he had to move from step to step by careful study and had no temptation to jump to conclusions.

What, it may be asked, was the net result of all this labour and ingenuity? To put it at the lowest, the interpreters produced order out of chaos, so that when they had done their work nearly the whole of the enormous mass of scripts fitted into a coherent pattern of which they had no conception when they started their labours—*nearly* the whole, that is to say, apart from exhortations to the automatists and interpreters and discussions as to experimental methods, which of course lie outside the pattern; and even when these are excluded there remain passages where the interpreters confessed themselves unable to discover a coherent meaning.

It would of course be easy enough for an unscrupulous interpreter to select tit-bits here and there from so large a mass, arrange them arbitrarily, interpret them according to the caprice of the moment, and thereby obtain any pattern he pleased. Those, needless to say, were not the methods of Balfour or Piddington or any of their fellow-workers. That so many pieces of the puzzle fitted neatly into place to produce an elaborate design incorporating many details that, considered by themselves, are extremely odd, seems to me strong evidence that the design really is

there, and is not the product of the interpreters' fantastic ingenuity.

Nor can it be assigned to the normal knowledge of each others' scripts acquired by the different automatists, e.g. through reading *Proceedings*, where several scripts were from time to time published, nor yet to rational inference based on such knowledge. Of the principal members of the group 'Mrs Holland' (Mrs Fleming) died without knowing even the main outlines of the pattern; Mrs Stuart Wilson does not know them yet; Mrs Verrall only learnt them from Piddington after most of her scripts had been written, and was with difficulty persuaded to accept his account; and my wife was equally hard to persuade when informed after her own scripts had ceased.

The choice seems to lie between three hypotheses, all paranormal, separately, or perhaps in some combination of them. To begin with the hypothesis involving the smallest departure from views acceptable to most psychical researchers, it might be argued that some person consciously or subconsciously designed the pattern and contrived to distribute it by telepathy among the members of the group, each of whom reproduced in his scripts the portion allotted to him. Those who incline to this view generally cast Mrs Verrall for the principal part, as she was the earliest of the group, and had the literary knowledge sufficient for producing the pattern as we have it. On the other hand she had no normal knowledge of some of the facts woven into the pattern, and the pattern went on unfolding itself for many years after her death in 1916. It should be noted that even this hypothesis, which may be taken as the minimum worth discussion, postulates telepathy of a kind very different from anything established or suggested by quantitative or qualitative experiment.

This same difficulty attaches to the second hypothesis, which is that the pattern was created by the subconscious minds of the automatists, acting as a group with a collective character that persisted notwithstanding the death or retirement of some of its members and the accession of others. This meets the difficulty as to the development of the pattern after 1916, but raises problems of its own, particularly as regards the implication of collective constructive activity on a very large scale carried on entirely at the subconscious level.

The third hypothesis is that the pattern was devised by some intelligence or group of intelligences external to the group of automatists, and, more specifically, by a group of communicators including Sidgwick, Myers, and Gurney, with each of whom some of the scripts claim to originate. This was the view that after prolonged study all the principal interpreters accepted, although

several of them were not lightly persuaded to accept it. Piddington's natural scepticism was reinforced by a personal dislike, which he expressed to me, for the idea of surviving ; but this did not prevent his accepting the survivalist view which he maintained in his paper in Volume XXXIII of *Proceedings*.

It may be doubted, however, whether Balfour, Piddington, and the other interpreters would have been willing to devote the immense amount of time and labour that the elucidation of the scripts demanded, if the sole object of the scripts had been to establish the survival and identity of the ostensible communicators. That was only the first stage in the declared purpose of the script-intelligence. The declared ultimate purpose is the creation of a universal and durable order of peace between nations and between classes, in the promotion of which the communicating group is, with many other persons, represented as engaged. This, one need hardly say, is an ideal that has appealed to many people at many times, and occasional references to it might have been expected in the scripts of automatists whose outlook on life was that of the members of 'the S.P.R. group'. When found there, they would have called for no particular notice. What struck the interpreters as significant was the prominence given to this topic in the whole mass of scripts, the persistence with which it is dwelt on from their beginning in 1901, and the peculiar way in which it is woven into a pattern embodying various paranormal features, cross-correspondences, verifiable statements of facts not normally known to the automatist making them, and predictions of public and private events, of which some had in the interpreters' opinion been fulfilled.

All this seemed to warrant their belief that they were in touch with discarnate intelligences having power to influence the actions of living men and women and foresee future events, and using that power to promote an enterprise of immense importance for the welfare of mankind. Holding that belief they were willing to spare neither time nor labour year after year in clearing up every doubtful point connected with the scripts, and to put the result of their labours in an orderly permanent form, so that, if the predictions were fulfilled, it should be beyond dispute exactly what the predictions were, when they were made, and from whom they claimed to come.

'These all died . . . not having received the promises.' The interpreters never supposed that they would receive them during their lives. But Piddington, the last survivor of them by several years, told me in the autumn of 1950 that though he was sure that the predictions had been correctly interpreted, the continued deterioration in world affairs had for some time made him wonder

whether the communicators and their associates had not undertaken more than they could perform. No precise date for fulfilment is given in the scripts, but the suggestion is certainly made that a beginning at least would take place in the lifetime of persons living at the date of the First War. It seems to me desirable that this should be stated now, without waiting for the predictions to be fulfilled by the course of events, or falsified by the lapse of time.

As I have already said, this is not the occasion to attempt a final appraisal of the scripts, and when the occasion does arise the attempt should be made by someone with a fuller knowledge of the whole of the scripts than I can claim. But I have read carefully more than half of them and, so far as concerns the material I have read, I am prepared to accept the interpreters' construction of it as substantially correct, even as regards points where the eccentricity to be expected in any work of complex symbolism might appear to have been given very free scope. But just as in ordinary life two people telling the same story with every desire to be accurate will each tell it from a different angle, so with the automatists of 'the S.P.R. group'. For example, all the scripts claim to be inspired by persons who have survived bodily death, and there is general agreement among the automatists as to who these persons are, but with some of the automatists they are presented in a highly individualised dramatic form, with others the emphasis is on what 'they' say and do, on their collective and, perhaps, inter-personal activity. It has sometimes seemed to me that in this and in other connexions the interpreters have overstressed the *personal* aspect, and that the cause of this may have been that with both Balfour and Piddington their subtle powers of reasoning were not tempered by psychic faculties, so that, quick as they were to discern what was or was not significant in substance, they were prone to accept too literally the dramatic form with which the substance was clothed. I hesitate, however, to bring even this minor criticism against a piece of work on so great a scale as the interpretation of the scripts, and one carried out with so much toil and skill.

On the other hand, Piddington had in full measure two indispensables for psychical research, human sympathy and a sense of humour. Because of his sympathy and his obvious trustworthiness and discretion, whenever, as often happened, it was necessary to verify some personal allusion in the scripts, he had no difficulty in getting people to answer questions as to their private affairs or to allow him to inspect their confidential diaries. His keen sense of humour made him a delightful companion and correspondent as soon as one penetrated his natural shyness and the reserve that went with it.

The poor health from which he suffered for many years did not prevent his deriving much quiet enjoyment from country life, golf and music, all of which he found at Fisher's Hill. From the windows of his book-lined study there he could enjoy delightful views of garden, woodland, and down, and there, whenever scripts became a weariness to the flesh, he could console himself at the piano, which he played very well. But above all Fisher's Hill gave him the company of like-minded friends. The last few years of his long life were spent in his daughter's charming house overlooking the Bristol Channel.

ICHTHYOSIS TREATED BY HYPNOSIS

STUDENTS of psychological research are familiar with claims that conditions resistant to normal medical treatment have been cured or relieved by 'psychic' means, by religious faith, or by a combination of the two. Thorough investigation of these claims is much more difficult than would appear at first sight, but most experienced psychological researchers—and certainly the medical profession—would attribute the results to suggestion applied to functional disorders of psychological origin. This view is supported by the absence of evidence that a congenital condition of an organic nature has been cured or relieved by suggestion alone. It is for this reason that a recent report in the *British Medical Journal* has aroused such interest.¹ The following condensation is printed by permission of the Editor of the *British Medical Journal* and of the author, Dr A. A. Mason, Senior Registrar, Queen Victoria Hospital, East Grinstead.

The patient, a boy of sixteen, suffered from congenital ichthyosis ('Crocodile Skin'), a condition whose aetiology is unknown and which is regarded as resistant to all forms of treatment. 'The lesion', writes Dr Mason,

consisted of a black horny layer covering his entire body except his chest, neck, and face. The skin was papilliferous, each papilla projecting 2-6 mm. above the surface, and the papillae were separated from each other by only a very small distance, perhaps 1 mm. The papillae themselves varied in size from small thread-like projections

¹ A. A. Mason, M.B., B.S., 'A Case of Congenital Ichthyosiform Erythrodermia of Brocq Treated by Hypnosis', *British Medical Journal*, No. 4781, 23 August 1952, pp. 422-3.

on the abdomen, back, and flexor surfaces of the arms to large warty excrescences 5 mm. across on the feet, thighs, and palms. The small amount of skin which was visible between the papillae was also black, horny, and fissured. To the touch the skin felt as hard as a normal finger-nail, and was so inelastic that any attempt at bending resulted in a crack in the surface, which would then ooze blood-stained serum. In the skin flexures there were fissures which were constantly being reopened by movement and were chronically infected and painful. The ichthyosiform layer, when cut, was of the consistence of cartilage and was anaesthetic for a depth of several millimetres.

The condition varied in severity in different areas of the body, being worst on the hands, feet, thighs, and calves, and least on the upper arms, abdomen, and back. The skin on the face, neck, and chest appeared normal, although, as is shown later, it became papilliferous when transplanted to the palms. . . . The patients parents are alive and well, and there was no family history of ichthyosis. His birth was normal, after an uneventful pregnancy.

After treatment at various hospitals without avail, on 25 May 1950 skin from the chest was grafted to the palms of both hands by Mr F. T. Moore, consulting plastic surgeon to the Queen Victoria Hospital, East Grinstead, but within a month it had become indistinguishable from the rest of the affected skin. A second attempt at grafting the palms two months later produced the same result, with the added complication of severe contractures of the fingers. Sir Archibald McIndoe and other plastic surgeons who saw the patient agreed that further grafting operations were unlikely to be successful, and that no alternative plastic procedure was possible.

Under hypnosis on 10 February 1951 the patient was given the suggestion that the left arm would clear.¹ (The suggestion was limited to the left arm so as to exclude the possibility of spontaneous resolution.) 'About five days later', says Dr Mason,

the horny layer softened, became friable, and fell off. The skin underneath was slightly erythematous, but normal in texture and colour. From a black and armour-like casing, the skin became pink and soft within a few days. Improvement occurred first in the flexures and areas of friction, and later on the rest of the arm. The erythema faded in a few days. At the end of 10 days the arm was completely clear from shoulder to wrist. . . . The right arm was treated in the same way (see Figs. 1*a* and 1*b*),² and ten days later the legs (see Figs. 2*a* and 2*b*; 3*a* and 3*b*) and trunk were treated.

¹ Dr Mason informs us that the treatment was carried out by him.

² The photographs are not reproduced here.

The result of the treatment is shown in the report as follows :

<i>Region</i>	<i>Before Treatment</i>	<i>After Treatment</i>
Hands	Completely covered	Palms clear. Fingers not greatly improved
Arms	80% covered	95% cleared
Back	Covered, but only lightly	90% cleared
Buttocks	Heavily covered	60% cleared
Thighs	Completely and heavily covered	70% cleared
Legs and feet	Completely and heavily covered	50% cleared

During the first few weeks of the treatment, 'clearance of the affected areas was rapid and dramatic', but 'during the last few months there has been no appreciable change. There has, however, been no relapse of the improved areas over a period of one year.'

REVIEWS

FATHER THURSTON. A Memoir with a bibliography of his writings. By Joseph Crehan, S.J. London, Sheed & Ward, 1952. vii, 235 pp. 12s. 6d.

THE PHYSICAL PHENOMENA OF MYSTICISM. By Herbert Thurston, S.J. Edited by J. H. Crehan, S.J. London, Burns Oates, 1952. viii, 419 pp. 35s.

Padre Pio da Pietrelcina. By C. C. Martindale, S.J. (*The Month*, June 1952. pp. 348-57.)

When Fr Thurston died in 1939 the English Jesuits lost one of the most distinguished members of the Society and Roman Catholics in England their greatest expert in psychical research. He was a tireless worker and his first ticket of admission to the British Museum Reading Room was dated 1880. During the latter years of his life I used to meet him constantly as we both laboured under the dome, and we were often in consultation over controversial points, and sometimes combined in an innocent plot to persuade the authorities to buy some rare book which we both needed and thought should be added to the shelves.

Fr Crehan's memoir is an admirable sketch of Thurston's life and activities and throws a vivid light on the course of his mental development and outlook. His interest in psychical research was

early aroused, and in 1899 George Tyrrell wrote to him saying that Everard Feilding wanted him to go to hear the ghostly choir at Slindon since the S.P.R. was not welcome 'because they deal in devils'. Nevertheless, Tyrrell tried to persuade Thurston to join the Society at the same time as himself, but it was not till much later that he did so, and in 1921 read a paper on the phenomena of stigmatization.

In his discussion of Fr Thurston's many controversies Fr Crehan has, in the main, succeeded in being as objective as could be expected, although his lapse in printing the utterly unfair and untrue attack on the late Dr G. G. Coulton (p. 153) is to be regretted. It is true that the latter's clash with Fr Thurston was one of the bitterest moments of his controversial career, but his defence of Lea was doubtless influenced by his belief in Lord Acton's judgment and his opinion (which I did my best to shake) that Fr Thurston was far from being the doughty adversary that I knew him to be.

In his treatment of Thurston's psychical interests Fr Crehan is wary, although he reveals the fact, hitherto quite unknown to me, that Thurston visited Naples in 1925 to see the liquefaction of the blood of St Januarius, a phenomenon to which he and I had devoted a good deal of attention. For reasons which have never been explained neither he nor any other authority has, to my knowledge, attempted to deal in detail with the so-called exposure which was published in the *Hibbert Journal* in 1921, and all my recent attempts to obtain information from Catholic sources have met with no success.

Apart from a few minor errors, such as 'Douglas' Home, Mrs 'Woodhill' and that Fr Thurston joined the S.P.R. in '1919', Fr Crehan's biography is an excellent introduction to the work of a man whose book, *The Physical Phenomena of Mysticism*, to which I will now turn, is the most brilliant and fascinating account of the phenomena of the Saints ever written. In editing the work, however, it is a pity that Fr Crehan has not given the precise history and date of each of the papers of which this book is a collection, as since he is not sufficiently acquainted with the subject, it is often impossible to determine whether Fr Thurston omitted to discuss relevant material or whether such material was published after the paper itself had been printed.

The book itself is a mine of information on the alleged physical phenomena associated with various holy persons. In dealing with these extraordinary manifestations Fr Thurston displays a wide acquaintance with the different sources, and usually estimates the degree of credence to be placed upon them with considerable acumen. Some of his criticisms and exposures will come as a

shock to those unaccustomed to weighing evidence; and the objectivity with which he treats the material cannot fail to commend itself to the psychical researcher, especially since he states that some of the evidence as printed in the *Processes* is 'notably better attested' than any to be found in the *Proceedings* of the S.P.R.

Among the phenomena discussed and documented in this volume will be found levitations, the stigmata, telekinesis, fire-tests, bodily elongation, the alleged absence of *rigor mortis* and the usual signs of corruption, and some remarkable cases of living without eating. For levitation a number of instances are adduced, some of which are very difficult to explain away, although I find it hard to understand why Fr Thurston, in trying to show that these phenomena were still noted as occurring in the nineteenth century, should quote the quite unacceptable cases of Marie Baourdie (d. 1878) and of Maria della Passione (d. 1912), neither of which have, I think, the slightest evidential value.

Among all the remarkable phenomena recorded by Fr Thurston it is, to say the least, somewhat suggestive that many of the mediumistic phenomena regarded by psychical researchers as almost certainly fraudulent are scarcely to be found among those recorded by hagiographers, materializations being confined, so it would seem, to the very curious cases of the alleged multiplication of food, which phenomena Thurston thinks cannot be lightly dismissed, although he agrees that the evidence is often inadequate to sustain them.

To anyone acquainted with the works of such writers as Schram, Görres, Ribet, Poulain, or Saudreau, this book must come as a surprise. For it illustrates the changes in the viewpoint of intelligent and informed Catholics in these matters better than any work I know. For not only was Thurston both *chercheur et critique* in theology, as the head of the Bollandists called him, but a man who knew it to be his duty to make himself informed on what psychical research was doing undeterred by the ill-informed gossip about the devils and the S.P.R. It was this comparative survey which enabled him to treat many mystical phenomena as examples of conditions common in psychopathology and having nothing supernatural or miraculous about them.

It must be realized that, to the Catholic, the supernatural is of two main types: (a) the essential supernatural which is not open to scientific scrutiny and is the object of faith, and (b) the modal supernatural or more simply the marvellous. Thus the stigmata and various alleged diabolic manifestations belong to the modal supernatural in certain cases, the *fact* of which can be established by science, but of which the explanation is not to be sought on

material grounds. Thus, in order to determine the truly marvellous, all natural explanations *must* be eliminated. It is precisely here that we can see how grave differences of opinion are likely to arise in, for example, cases of diabolic possession where, to use the words of a recent writer in the *Etudes Carmélitaines*, 'le monde ecclésiastique n'est spontanément que trop porté, en cette matière, à une crédulité naïve', and where the imprudent and ill-considered action of certain priests is liable to do harm rather than good to the psychotic subject. Thus in the case of the stigmata, which psychical researchers have for long regarded as closely connected with hysterical and neurotic states (which conditions, in their relation to mysticism, had already been discussed by Fr G. Hahn in 1883), Thurston was insistent on the fact that hardly any cases are known where nervous disorders had not appeared before the development of the wounds, and indeed, he went so far as to say that he would like to hear of one case of a stigmatica who had no bad family history. In view of the prevalence of hysteria in the conditions under which women were formerly brought up, he pointed out how, during the last seven centuries, female stigmatics vastly outnumber the male, of which only two clear cases seem to have been recorded in which all the five wounds were externally observable. Although this disproportion is more than once mentioned by Thurston, it does not seem to have occurred to him that this is possibly due to the fact that the object of concentration and worship is male, and that it is this sexual element which is mainly responsible, a supposition supported by the cases of the very curious espousal rings which he describes in Ch. III, and by the fact that, at least in one case, a stigmatica, having happily married, was no longer stigmatized.

Although Thurston admits a number of instances of imposture among those with whom he deals, he has been clearly very careful to avoid any account of the more flagrant cases (such as the Holy Bambino of Bari) which were supported by high ecclesiastical authorities presumably for the benefit of those unable to detect or to expose the fraud.

In dealing with cases of stigmata prior to St Francis, Fr Thurston comments very unsympathetically on the English case of the early thirteenth century where Merkt claimed a genuine example of stigmatization, but which, as seems fairly clear, was a case of crucifixion and self-inflicted wounds, which is actually stated to have been so in the *Annals of Dunstable* and by Ralph of Coggeshall. That this is not the explanation in the modern case of Padre Pio seems to be certain, judging from the admirably concise account by Fr C. C. Martindale in the June 1952 issue of *The Month*. The extreme caution displayed by the Holy Office

in this case is clearly due to the influence of more modern views of the nature of the stigmata and their close connexion with psychopathological factors operating in the subject. What must strike the non-Catholic as so odd is that it does not seem to have occurred to some of the ecclesiastical authorities that, just as it is affirmed that the devil can profit by functional disequilibrium to gain influence over a sufferer and, indeed, cannot exert his power without such mental and nervous trouble being present, so God might exert His power in the formation of the stigmata only in persons whose psychological make-up lends itself to such Divine interference. From this point of view the stigmata of Padre Pio can be clearly classed as supernatural. This caution on the part of the Holy Office can, perhaps, be linked with the clarification of the views of Fr Thurston concerning the stigmata which were due to his study of the alleged production of such marks on the Austrian peasant woman by the Lutheran physician, Alfred Lechler, in 1932. Apparently Thurston accepted this story as conclusive proof that 'stigmata' could be produced solely through suggestion, and his acceptance is yet another indication of his increasingly credulous attitude when examining medical testimony concerning unusual physiological and psychological phenomena. This tendency, now becoming increasingly common among well-informed Catholics, is due to the fact that they deliberately cut themselves off from any *practical* experience of the phenomena with which they are dealing. Relying on printed sources they have little or no acquaintance with the true nature of the facts described, and thus are apt to credit tales of events which, had they actually been present, they would at once have seen had but little resemblance to the description given later by enthusiastic and untrained witnesses.

Towards the end of his life Fr Thurston became more and more prone to believe the stories of spiritualistic phenomena printed in presumably reputable journals, and my oft-repeated words of caution merely seemed to him to be the product of an unreasoning scepticism. With his acceptance of much of the Margery phenomena, even *after* the thumb-print controversy, he showed that even his critical mind had failed to resist the barrage of suggestion and display of pseudo-science which was becoming one of the most marked features of psychical research and which, once understood, was to lead many orthodox scientific men to doubt the validity of almost everything that came out of the parapsychological laboratories. That Fr Thurston failed to see through what lay beyond was another proof, if any were needed, of the goodness, simplicity, and faith which inspired him. His book is not only the achievement of a scholar and critic which, for the first time,

offers the evidence for the phenomena of the saints in an acceptable form, but the work of a very human and lovable personality to whom I am grateful for having been allowed to share, in however slight a degree, in the controversies and problems to which so many years of both our lives have been devoted.

E. J. DINGWALL

TELEPATHY AND SPIRITUALISM: personal experiments, experiences and views. By J. Hettinger. London, Rider, 1952. 150 pp. Illus. 16s.

This book covers a number of substantially unrelated topics and includes much that has already been published. Ch. VII, for example, gives Dr Hettinger's views on 'telepathy *v* clairvoyance', which were printed in S.P.R. *Proceedings* for June 1946. Ch. XI is a record of mediums' impressions collected by Dr Hettinger relating to the as yet unopened envelope left by the late Sir Oliver Lodge. Ch. X gives Dr Hettinger's views on Spiritualism, together with his account of the personal experiences with mediums that have made him a believer in communication with spirits. In Ch. I there is a discussion of the 'handicaps in the search for truth' deriving from 'the limitations of the field of mental vision' and 'our personal mental worlds'. A prominent feature of this chapter is the 'integrated diagram' (circular) of 'our personal mental worlds' in which there are assigned positions (segmental) to the realms of 'nature', 'mind and spirit', 'human relations', and so forth. It is interesting to juxtapose this with the comment in the following chapter that Whately Carington had a weak spot for philosophy.

Several chapters are a summary of Dr Hettinger's previously published investigations in object-reading. It is unnecessary to say much about this, since his two books on the subject, *The Ultra-Perceptive Faculty* (1940) and *Exploring the Ultra-Perceptive Faculty* (1941), have both been reviewed already in this *Journal*, and in addition the whole research has been analysed by Christopher Scott in S.P.R. *Proceedings* for November 1949. Briefly, the method is to present mediums with personal objects and collect impressions from them which are supposed to relate to the owners of the articles. Dr Hettinger adopted various control devices. For instance, he would pair all the items given by the medium in connexion with one article with items given in connexion with other articles in order to find out if the owner would be able to identify that member of each pair which was actually intended to relate to him.

Experiments of this type are not as easy to carry out objectively

as they appear to be at first sight, and the methods of Dr Hettinger made error likely. In some cases it seemed that the control items were not of the same quality as the items with which they were paired. Another difficulty was the possibility of bias in selected recording, since it was Dr Hettinger himself who sat with the mediums and wrote down their statements, and in most cases he knew the owners of the objects. There were also many elementary errors in the statistical evaluation tending to exaggerate the significance of the results, which were already doubtful on grounds of faulty experimental method.¹

In Dr Hettinger's later work, the owners of objects looked at magazine illustrations at the time of the tests, and the mediums' impressions were supposed to relate to the content of these target pictures. Dr Hettinger considered that the correspondences obtained were self-evidently more than chance expectation, so that no controls were needed. The coincidences illustrated in *Exploring the Ultra-Perceptive Faculty* were certainly striking and likely to convince an uncritical reader, but then they had been selected from a large mass of material. The whole matter depended upon Dr Hettinger's subjective judgment, and that this was not altogether to be trusted is obvious because he seemed unable to appreciate that some at least of the correspondences would have occurred in the absence of telepathy. When he was prevailed upon by the American S.P.R. to have some of his data, together with some control material, scored by an independent judge, the scores on the control material were no different from the scores on the rest of the data. Christopher Scott also carried out an experiment with Dr Hettinger and, unknown to the latter, he substituted control pictures for some of the illustrations that had been used as targets in the usual tests. Dr Hettinger himself did the scoring, and again there were as many correspondences on the control pictures as on the others. A reasonable conclusion is that Dr Hettinger judged wrongly, and that the results were no different from chance expectation.

In Ch. VIII Dr Hettinger describes a new development, the attempt to transmit a code message by the use of his picture tests. A prepared set of target pictures is made to stand for the code words, and these are concentrated upon by an agent in the order representative of the words of the message. As before, the sensitive, at a distance, handles an object belonging to the agent. Her impressions are recorded by the experimenter, who notes any correspondences with the known target pictures and, from these

¹The best available methods for the conduct and evaluation of object-reading tests are described by Pratt and Birge, *J. Parapsychol.*, December 1948.

correspondences, attempts to deduce the order in which the targets were looked at and thence the words of the message. This, of course, is an example of the use of pre-arranged targets in place of free material, a method which in another chapter Dr Hettinger condemns. It is a method which gives results that can readily be assessed statistically, but the figures are not quoted. Dr Hettinger tells us only that this method, and various modifications of it, all proved inadequate for the transmission of messages. As the evidence suggests that the previous results from picture tests were on a level with chance expectation, this conclusion is not surprising. Dr Hettinger gives a number of plausible rationalisations to explain his frustration.

It is a tragedy that years of effort were wasted on these faulty experiments. Chapter II reveals what is mainly responsible for their failure: an attitude of puerile animosity towards other investigators has prevented Dr Hettinger from seeking friendly advice at an early stage and so forestalling criticism. Instead he maintains his lone though vulnerable eminence by simply ignoring other people. It is typical of him that beyond the comment (p. 27) that other investigators have criticised his work as he criticises theirs, he gives the reader not the slightest hint that much of his research has been demonstrated to be ill-founded. His opinions of the research of others make droll reading. He has a bee in his bonnet about statistics, which he obviously does not grasp, so that he finds himself unable to accept the results of card-calling tests without an empirical control of randomly picked cards to demonstrate the level of chance expectation (p. 24). If only he had been as particular about his own picture tests! He condemns both Rhine and Soal for using a fixed number of symbols instead of 'free' material like drawings, but he is dissatisfied with Carington's work with drawings because the results were statistical and the subjects were not specially selected sensitives. As for Dr Rhine's PK hypothesis, he is 'very much surprised that the idea was mooted at all . . .' (p. 26). I have a similar sentiment about Dr Hettinger's hypothesis (Ch. VII) that cases of apparent precognition are due to the intervention of spirits. According to this theory a spirit anticipates or brings about an action in individual A. and at the same time gives individual B. an apparently premonitory impression of A.'s future action.

I have deliberately left to the last consideration of Ch. IX, because I believe it is the most important. In it there is a description of an ingenious new experiment designed to demonstrate the long-distance telepathic transmission of emotions. An agent is subjected at irregular intervals to painful or startling stimuli. The sensitive is connected to an apparatus for recording the psycho-

galvanic reflex—that is, changes in electrical resistance of the skin which coincide with emotional reactions. Deflections of the galvanometer corresponding in time with stimulation of the agent are held to indicate that the sensitive, subconsciously at least, is reacting to the agent's emotion.

The experimental design is amenable to a simple statistical control. The agent's stimuli are always given at exactly full minute intervals from the moment of commencing. On the average there is a stimulus at one in three of the minute intervals throughout the experiment. Whenever there is a galvanometer deflection within a fifteen-second range around the minute intervals this is counted as a reaction. It is stated that reactions *always* occur more frequently at the minute intervals when there is an agent stimulation than at the minute intervals when there is no stimulation.

No detailed account of the experiment is given, and it may be that randomisation of the times of agent-stimulation and other obvious precautions may not have been taken. If so, the project would be just one more example of a typical Hettinger experiment, excellent in conception yet faulty in execution. Personally I am optimistic. It would be an outstanding advance if this should prove to be the long-awaited method of obtaining accurate ESP responses by circumventing conscious inhibitions. I am sure we all hope to find in the full report the fruitful and valid consummation of his labours which Dr Hettinger's persistence richly deserves.

D. J. WEST

WHERE TWO WORLDS MEET : the verbatim record of a series of nineteen séances with John Campbell Sloan, the famous Glasgow direct voice medium. By Arthur Findlay. London, Psychic Press, 1951. 624 pp. 1 plate. 12s. 6d.

Do spiritualists realize how much harm is done to their cause by a claim, such as that made by the author on the cover of this book, that 'No more conclusive evidence of survival is available' than the reports of sittings he has printed? If such a claim could be substantiated, the case for survival would be very much weaker than I believe it to be.

John Campbell Sloan, who died in May 1951 while this book was being printed, had practised as a medium for fifty years, and Mr Findlay's acquaintance with him goes back more than thirty years. It would naturally be distasteful to any living medium to be forced into competition with another medium who has recently died, but as Mr Findlay has made this claim it must be pointed

out that the communications recorded in this book were produced under much less rigorous conditions than those recorded in the case of several other mediums both living and dead. The sittings with Sloan were held in a circle of about half a dozen, most of whom on every occasion appear to have been regular sitters living in the Glasgow area. He could hardly have failed in course of time to have acquired normally much information about the regular sitters and their friends and relations, living and dead. The same was true of the Piper mediumship in its early stages, and it was to avoid prejudicial inferences that might be drawn from this fact that Mrs Piper was brought over to England, to sit with persons about whom she had no previous knowledge. After her return to the United States she was confronted with a very large number of sitters quite unknown to her and often introduced anonymously. Much more evidential value is to be attached to communications received through a medium giving sittings to single sitters who are frequently changing than to communications given to a more or less constant circle.

The sittings recorded in this book took place at various times between April 1942 and July 1945, when Sloan was already an old man. Several times during the sittings he complained of feeling tired, and it is probable that, whatever his psychic powers may have been, they were already in decline. It is accordingly the sitters, who seem to have been very easily satisfied, rather than the medium, against whom criticism should be directed. Take, for example, the incident reported on pp. 148-9 when the medium said he had been looking at a photograph of the husband of Mrs Lang, and that it was not very clear. A voice here interrupted, 'Yes, you lost the negative and it had to be taken off an old photograph,' and persisted in this assertion notwithstanding Sloan's denial of all recollection of the incident. Mrs Lang, one of the regular sitters, then exclaimed, 'Yes, that is right, Mr Sloan. I remember you lost the negative and hunted all over for it. Who is it who knows all about photos? It must be someone who knows me very well.' So of course it was, as the incident had at one time been perfectly well known to Sloan's conscious mind, and the interruption is at best a dramatization of a lapsed memory.

It is stated that Sloan frequently spoke in foreign languages 'which sometimes could not be understood', but we are given very few examples of foreign speech, whether understood or not. Twice we are given two words of Gaelic, a language of which Sloan probably knew something as his mother came from the Highlands. There are four words of Latin, 'Ars longa, Vita brevis,' a sufficiently familiar tag, and a Danish communicator, together with some broken English, spoke two words which are

apparently claimed as Danish. Another communicator 'started to speak fluently in French', but we are not told whether any of the sitters took part in the conversation, and if so whether the communicator responded intelligently to their remarks, or whether any of the circle knew French well enough to judge if the communicator's language and pronunciation were correct. Sloan had at two periods of his life followed the sea and may very likely in that way have acquired a smattering of the languages of two countries with which we have as close commercial relations as France and Denmark. What a pity that the Circle did not use a recording machine so that the Danish and French could be played over to persons familiar with those languages, and the Red Indian, East Indian, and African Guides have been invited to make records of their own languages which could have been submitted to experts!

Two among the communicators gave the names of W. E. Gladstone and J. M. Barrie, who in this life were notable for intellectual gifts and powers of expressing themselves which they appear to have lost when shuffling off this mortal coil (see pp. 119 and 176). In justice to Sloan it may be said that some of Mrs Piper's Controls represented even less worthily the great names they claimed.

Generally speaking, while the records are satisfactorily full as to what was said at the sittings, the reader looks in vain for the wealth of annotation by the sitter which gives value to the records of Mrs Piper, Mrs Leonard, Miss Cummins, and other mediums of their type. The absence of details of this kind puts this book definitely on a lower level than the published records of these mediums.

A characteristic of the Sloan mediumship, which distinguishes him from these other mediums and appears to Mr Findlay to be of crucial importance, is the association of physical phenomena, such as the movement of trumpets, with the communications. The sittings were held in complete darkness and Sloan was not subjected to either manual or mechanical control. Mr Findlay complains that the scientists of Glasgow University paid little attention to Sloan's phenomena and did not employ infra-red photography. One may fairly ask why Mr Findlay and his friends did not employ this technique themselves, or if they felt they had not the expert knowledge, why they did not pay somebody to attend the sittings and do the necessary things on their behalf. But is there any reason to suppose that Sloan would have agreed to this? The infra-red telescope was not available for general use at the time these sittings were being held, but it is not irrelevant to note that since it has become available physical mediums are very

shy of it, although it is well known that the use of it makes unnecessary some of the other techniques of control which mediums profess to find irksome. In the points, therefore, in which Sloan's mediumship during the years 1942-5 can be compared with that of such mediums as have already been mentioned, it falls far short, while its distinctive features are, owing to the absence of control, of no evidential value.

On pp. 342-3 Mr Findlay makes an attack on the Society which calls for a brief comment. He says that in 1925 he suggested to Sir William Barrett to put before the Society a proposal to investigate Sloan in London, Mr Findlay paying the expenses. He continues, 'Sir William went to the next Council meeting and put forward my offer. Much to his disappointment the Council turned it down with neither an explanation nor an expression of thanks, and, when he told me of its decision not to investigate Sloan's mediumship, he was not only disappointed but angry. That ended the matter and nothing was ever done. My reaction was to resign my membership of the Society.' This is a charge against the Society of failure to perform its duty as an investigating body, and also of discourtesy to Mr Findlay. As regards the second and minor charge Barrett never laid the proposal formally before the Council, although he consulted some of the Officers. I remember very well hearing of the proposal, doubtless as being then Hon. Treasurer. What report Barrett gave Mr Findlay of his discussion with the Society's Officers I do not know, but if there was any failure of courtesy it was certainly not on the part of the Officers or Council.

The other charge is a more serious one. In bringing it Mr Findlay should in candour have mentioned a very material condition that he attached to his proposal, namely that the Society should engage Sloan as caretaker at Tavistock Square. While the Officers of the Society had no reason to regard Sloan as personally untrustworthy, they did not consider it good policy to allow a professional or semi-professional medium the free run at all hours of the day and night of premises where a large amount of highly confidential information was (and is) kept. Much of this information has been entrusted to the Society precisely because it could only come into the hands of officials specially charged with the duty of secrecy. Any suspicion that a medium has had access to material subsequently referred to in communications received through him or through any other medium goes far to discredit any evidential value that might otherwise attach to them. The best mediums realize that it is to their interest that no such cause for suspicion should occur. In this respect they show a better appreciation of evidential standards than Mr Findlay

who, when informed of the reason why his proposal had been declined, light-heartedly brushed it aside.

Incidentally it may be mentioned that this incident occurred in 1923, not 1925, when the Society had no vacancy for a caretaker, and that Mr Findlay's reaction to resign did not take effect until 1932.

W. H. S.

THE DEVIL IN MASSACHUSETTS : a modern enquiry into the Salem witch trials. By Marion L. Starkey. London, Hale, 1952. 269 pp. Illus. 18s.

This 'modern enquiry into the Salem witch trials' fully deserves the claims made for it by the publishers. It certainly 'combines a narrative that has the pace and excitement of a novel' with 'an authentic history of the Salem witch trials', and if the application of 'modern psychiatric knowledge to the witchcraft hysteria' is not very elaborate or profound, it is quite sufficient for the purpose in hand. The primary purpose of the book is to present a clear picture, as accurate as careful documentation can make it, of the intense but short-lived outbreak of witch-hunting which swept through Salem village and some of the surrounding districts in 1692. The phenomena, wholly unintelligible in that age, reveal upon a tiny scale, for only twenty victims in all were put to death, the astonishing effects of mass hysteria, such as held Europe for three centuries at a cost of hundreds of thousands of lives. But at Salem they took place in a community rigid alike in Calvinist theology, in Puritan morality, and in a real desire for justice. The very forces which produced the outbreak by driving a group of adolescent girls into the self-display of hysterical convulsions, in which they named the alleged witches who were supposed to be tormenting them, were the forces which speedily led to its control. The whole story is fully documented by court records as well as by the contemporary writings of Cotton Mather and others immediately concerned, and is here admirably told.

From the point of view of parapsychology the most striking fact is the obvious honesty of all concerned, except perhaps the slave Tituba who first awakened the hysterical symptoms in the children who formed the original 'young people's circle'. Once the fever of suspicion had spread every sort of story was believed, 'shapes' were reported as acting anywhere and everywhere, and the visions of hysterics completely outweighed the ordinary evidence of sober people. One of the strangest figures of all is the grim Chief Justice Stoughton, who seems to have been wholly sincere and wholly free from hysteria, devoted to the one purpose of exter-

minating witches by strict process of law, and yet quite unable to see the complete worthlessness of the evidence upon which his judgments were based. And the fundamental religious strength of a community has never been more fully demonstrated than by the public pardoning of Ann Putnam, no longer a hysteric, and her restoration to her place among them by the very relatives of those whom her testimony had sent to the gallows.

But whether the ghost of Mary Esty really appeared to Mary Herrick, and so won the day, the reader must judge.

L. W. GRENSTED

BORDERLANDS OF SCIENCE. By Alfred Still. London, Rider, 1952. 276 pp. 20s.

This work is in the main a potted history of the wax of science and psychology and the wane of superstition, with the theme extended very naturally to include a brief survey of the various phenomena which come under the heading of psychical research. Unfortunately, one is left with the impression that most of the information could have been equally well obtained by studying jointly an encyclopaedia and a *Who Was Who*. But if Mr Still adds little of his own to the general discussion, he does at any rate point a very clear way to other sources of information.

He makes a bold, interesting, and praiseworthy effort to show the error of science in rejecting out of hand those problems at present beyond its normal specialized scope. But though repeatedly concerned over the fallibility of human judgement, the author would nonetheless seem to have made up his own mind on the strength of evidence that is left quite unexplained. Thus, when we are told (p. 102) that 'Many careless people are sceptical about a future existence . . .' we search unrewardingly for an expansion of view that Mr Still has been anything but careless to hide.

We cannot fail to appreciate his obvious sincerity, however; nor yet the unusual courage he displays in squirting Spiritualists, scientists, astrologers, believers and unbelievers from the same bottle of ink! Throughout this book the desire to be objective is certainly unmistakable.

In spite of the fact that an interesting chapter on Body, Mind, and Memory shows the author to be not entirely unfamiliar with the complex problems of subconscious activity, the practical importance of intuition is grossly overrated. However pure the basic intuition may be, it can rarely if ever emerge into reflective awareness without being modified by a very fallible subconscious. While, therefore, we can study the results of supposed intuition, the basic factor is itself a matter at present beyond the scope of

final objective pronouncement. In other words, intuition is largely a private matter, and is far too unreliable to serve as a self-sufficient aid to the establishment of any 'universal truth'.

Perhaps the author's failure to realize this explains his aversion from controlled experiments in ESP, and his complete neglect of Dr Soal! Thus, regarding quantitative experiments we read that 'Conclusions drawn from such data are not likely to impress the scientist, especially if he is also a mathematician. Yet several recent investigators have adopted similar methods in the belief that the analysis of the accumulated data provides better evidence of a telepathic faculty than the many plausible stories of spontaneous thought transmission' (p. 217). But overleaf, on the one page devoted to Dr Rhine, Mr Still has this to say about the ESP tests: 'These experiments also go a long way towards shattering the generally accepted doctrine that nothing can enter the human mind except by way of the recognized bodily senses.'

We are led to assume, therefore, that in the author's opinion, only where the experimental and statistical method produces relatively quick, favourable, and spectacular results is it admissible!

The range of subjects in this book is, of course, such that perhaps one could not reasonably expect a combined historical and critical approach in 276 pages. We are introduced to witchcraft, magic and superstition; to faith-healing, time, and the subconscious; to the divining rod, levitation, and poltergeists; to Copernicus, Galileo, and Newton. So, no doubt inevitably, we are left in the end to form our own acquaintanceships.

As I have said, this work is mainly a potted history. And judged as such it can be readily recommended to those who are just beginning to take an interest in the manifold problems of the 'paranormal'.

THOMAS GREENWELL

JOURNAL OF PARAPSYCHOLOGY. Vol. 16, No. 2, June 1952.
Durham, N.C., Duke University Press. \$1.50.

The central place in this number of the *Journal* is taken by an article by Professor Rhine on 'The Problem of Psi-missing'. The fact that some subjects score consistently below mean chance expectation while others drop below mean chance expectation at a late stage of experimenting is a challenging observation, both because of its practical inconvenience and its possible theoretical implications. Professor Rhine has made a scholarly study of the experimental evidence on the subject and focusses attention on the problems it raises.

Dr Gertrude Schmeidler has made a study of 'ESP Scores of Patients suffering from General Concussion'. She finds that eighteen concussion patients score better on ESP tests than the general average. She suggests that this may not be a direct result of the concussion, but of the relaxed and uncritical attitude which results from concussion. Every study that suggests how scores in ESP tests may be improved is to be welcomed, although a less drastic way of inducing a relaxed attitude than knocking our subjects on the head must still be used in our experimental work.

There are reviews of Ducasse's *Nature, Mind, and Death* by Professor H. H. Price and of Rawcliffe's *The Psychology of the Occult* by Dr D. J. West.

R. H. THOULESS

JOURNAL OF THE AMERICAN SOCIETY FOR PSYCHICAL RESEARCH.

Vol. 46, No. 3, July 1952. New York, A.S.P.R. \$1.50.

Communications on psychological research from South Africa are a rarity and so welcome. May Bell of Rhodes University, Grahamstown, in 'The Normal Function and Manifestation of Psi', postulates a trisection of the self into 'Con', the conscious self, 'Uncon', the Freudian Unconscious, and Psi (Thouless-Wiesner). By definition Uncon and Psi are mutually exclusive.

Some of the arguments adduced by the author to support her postulated framework seem to me to beg the question. Thus, of dreams: 'Uncon's repressed desires are, apparently, the motive of the dream, but what tidies it up into a nice unshocking story that Con will cheerfully accept? Hardly Con, whose feelings are being saved, scarcely Uncon, which we are led to think of as wholly concerned with getting its desires across to Con. There seems to be a censor, with some dramatic ability; if this is neither Con nor Uncon, unless we are to invent further entities, must it not be psi?'

The interesting paper on precognition by C. T. K. Chari which appeared in the *Journal* of the S.P.R. for November-December 1951, is reprinted in slightly expanded form, and Mrs Allison contributes a memoir of J. G. Piddington.

An exchange of letters between Dr Rhine and two Yale University psychologists is reprinted. Sheffield and Kaufman, the Yale men, were reported in the *New York Times* (30 March 1952) as having carried out a PK experiment by a Duke technique using both human and photographic recording. According to Sheffield and Kaufman, the camera showed that believers in PK tended

to make recording errors which inflated the scores while disbelievers understated the scores. Rhine takes them to task, claiming that their criticisms are irresponsible, inviting them to an open forum to discuss the matter, and offering to publish their paper in the *Journal of Parapsychology*. Sheffield and Kaufman decline the invitation to a public debate, but promise their paper for publication.

D. P.

CORRESPONDENCE

THE INTERPRETATION OF ESP EXPERIMENTS

SIR,—I wish to draw attention to a point regarding the logical interpretation of ESP experiments, which appears to be generally overlooked. The procedure used when we infer a causal relation between two phenomena from a statistical experiment is as follows. A set of experimental objects (say, plots sown with oats) is divided at random into two or more subsets, each member of one subset being treated in one way (say, with a certain fertiliser) and each member of another subset in another way (say, with no fertiliser). Then we measure every member of both groups in respect of some chosen character (say, yield) and, treating the two sets of measures as statistical 'populations', we can calculate the probability on the given data that the differences between them are due merely to the random segregation of the original set of objects into two groups. If this probability is small enough, we feel confident that the differences are not due to this cause *but to the only other difference we have made*, that is, to the different treatments. Provided the randomization has been properly carried out (and the process involves various technical complications varying from one experiment to another) we can attach a definite probability to the validity of our conclusions.

Parapsychological experiments do not in general conform exactly to this type. Their material is not divided into two groups, of which one serves as a control; the place of the control is taken by an imaginary set of results obtained by purely random selection, and this selection is not really carried out but represented in the calculations by the known statistical parameters of such series. We have in effect two series of guesses, one made by a conscious subject supposedly trying to obtain correct hits, the other by an imaginary machine working with mathematical perfection as a

randomizer. The positive results which have been obtained prove that these two methods of guessing produce different results, and in particular that a subject consciously attending to his guesses will very often not be guessing at random even if he have no sensory clue to guide him. That is obviously an interesting and important conclusion. But it involves no more than an association of phenomena, and gives no hint as to the causal relations. For this, a proper control is needed.

The mathematical control in these experiments lacks at least three factors present in the 'treated' runs : (a) the experimental situation, (b) the act of conscious guessing, and (c) any special mental faculty which may be involved. It is illogical to dismiss (a) and (b) lightly ; the anecdotal evidence rather suggests that the experimental situation may influence the results, and it needs proof (however plausible it may appear) that a conscious subject *can* make a series of guesses statistically uncorrelated with the 'targets'. A more radical possibility which really ought to be ruled out by formal experiment is that the non-random runs are primary and produced by chance, but that they in some manner attract ESP experimenters ; put thus, it sounds rather fantastic, but it is really no more contrary to our accustomed habits of thought than is precognition, widely accepted as an alternative but narrower hypothesis.

I am unable to suggest any means of deciding these questions experimentally in the case of psychognosis ; but the case is better in psychokinesis, for here one can make a genuine control in which the attention of the subject is the only missing factor. If one makes a hundred runs of dice-throwing provided with random targets, and if ten of these are selected beforehand at random and a psychokinetic subject called in to influence these only, one could tell from the results if the efforts of the subject were an objective 'cause' of departures from randomness, for if it were not so the results of the 'treated' runs would not differ significantly from those of the 'controls'.

It is already proven that phenomena occur which do not fit in with our customary habits of scientific thought. That being so, we must give up those habits, to the extent of abandoning common sense in the framing of our hypotheses. Until we have reformulated guiding principles of thought, nothing is too absurd to be worthy of formal disproof ; for we may be sure that there are still many absurdities which will turn out to be true.

A. F. PARKER-RHODES

Magdalene College, Cambridge

QUALITATIVE MATERIAL AND THEORIES OF PSI
PHENOMENA

SIR,—In the May-June 1952 issue of the *Journal* of the S.P.R., W. H. Salter has a note under the above title. In it he refers to my last article in the A.S.P.R. *Journal*, January 1952, entitled 'The Psychodynamics of Spontaneous Psi Experiences'.

Mr Salter very wisely warns against what he calls 'a source of possible misconception'. He is fearful that my emphasis upon *experiencers* rather than *experiences* may lead the reader to draw the conclusion that the *experiences* need not be investigated. This would be equally unscientific, in my opinion, as the current trend to emphasize the *experiences* to the exclusion of the *experiencers*.

A scientific approach to a problem demands that all known parameters be investigated as thoroughly as possible. What happens, unfortunately, is that the availability of materials and hypotheses, or the personal interest or prejudice of the investigator, permits one or another aspect to be emphasized or excluded. The trend in psychical research has been steadily away from human beings. It was against this tendency to ignore the human personality and to see only the 'phenomena' (and quantitative data) that I wished to take a strong stand. I never suggested or intended that research work along other lines be stopped. We are grateful, therefore, for Mr Salter's expression of concern.

I am, however, not in agreement with the very tendency against which I warned even when it appears subtly in Mr Salter's statement. The preference for the *experience* rather than the *experiencer* is insinuated by Mr Salter in his value judgment that the psychological analysis must be held up until 'the laborious first step be taken of sorting out the material under examination into veridical and non-veridical'. Efforts at such a sorting process are time-consuming and, at present, result in findings that are of dubious reliability, at best. Moreover, the supply of research funds and personnel is very limited. Mr Salter's recommendation, therefore, would in effect perpetuate the barren direction of contemporary psychical research.

EMANUEL K. SCHWARTZ

New York.

CONDITIONS FAVOURABLE TO ESP

SIR,—It is, perhaps, worth reminding ourselves from time to time that the limitations so far imposed by laboratory technique on research into the process of ESP are not unlike those suffered by observers of wild animals confined in a cage, and also that

spontaneous cases—even though some are chance coincidences or spurious—may give us clues to Whipsnade conditions for experimental percipients.

The following incidents, for example, three of which occurred during the last few weeks, demonstrate conditions which have synchronized with apparent ESP in my own family for many years, and which are well known elsewhere. The difficulty of incorporating them into planned experiments does not alter the fact that they seem to be soil in which ESP appears to flourish.

In the first four cases there was sudden emotion on the part of the apparent agent. They might almost be called trivial crisis cases. Last month I went, very exhausted, to catch a bus to our country cottage. It was, for the first time, full up, and only after much waiting and three changes did I arrive four miles, instead of half a mile, from home. I felt far from well and telephoned anxiously to my husband to fetch me, but, to my great disappointment, he was playing golf. Fortunately a stranger gave me a lift to the turning where I usually got out of the bus, and, as we arrived, my husband drove up. 'What an extraordinary coincidence,' I said, for there was no bus due, I had in any case not said when I meant to come down, and I had never before wanted him to meet me when going to the cottage, and he had never done so. 'Not at all,' he replied. 'As we finished our game, I felt you wanted me, so I refused the drink offered by my opponent, saying that if I hurried I should just catch you, and I came at once. I knew you would be here.'

Last week, again, my husband went away to golf, inadvertently taking the key of our son's car in his pocket. The latter was greatly inconvenienced, and I, rather distressed, suggested telephoning the Club. He did so at once, and the caddie-master replied, 'I can see the Colonel coming in now.' My husband, on being called, said that he had come in on purpose, feeling that I wanted him on the telephone. (He does not do this on occasions when I do not want him.)

A wave of feeling, however trivial, seems to do the trick in both directions between my husband and myself. On one occasion I felt quite suddenly that he needed me, when I was on my way home, having left him at an office for an appointment. I turned the car, rather inconveniently in a narrow country road, and went back, to find him leaving the building, having heard that his appointment had fallen through and much annoyed at being without transport home. On another occasion, during the war, I was sitting quietly at tea with my mother in the country, when I felt suddenly convinced that my husband had no torch for his walk home from the station in the blackout, on a very dark, unpleasant

winter's night. So, in spite of my mother's indignant protests that he had never before forgotten his torch, and why should I imagine he had done so now, I walked with a torch to the station. 'Thank goodness you've come,' said my husband. 'My torch went wrong as I left the Ministry.' Chance coincidence apart, this seems more likely to be a case of response to my husband's annoyance at the prospect of an unpleasant walk in the dark than of precognition, since a moment later a friend offered us a lift home and the torch was not needed.

The following case reminds us of the value, both of a really carefree attitude and of an encouraging atmosphere. An experienced member of the SPR handed me an envelope, saying casually, 'Can you tell anything from handwriting?' I said, no, but he insisted on my trying, *as a joke*. 'Is it a man or a woman?' he asked. The writing looked very feminine, but I had a sudden conviction that it belonged to a man, an extreme homosexual, and I gave a fairly detailed description of a type I thought most unlikely to be corresponding with my questioner.¹ He laughed, but said no more than, 'Go on. You haven't put a foot wrong yet.' Extremely pleased with myself, I made a few more statements, and I seemed to hear an interior voice make a special point that I should mention that the man liked budgerigars. I was then told that all I had said had been correct and that the man was very fond of exotic birds and constantly painted them. Next day my questioner telephoned to say that he had had another letter from the man I had described, saying that he had just bought a pair of budgerigars. I feel pretty sure that I should not have got the second wave of facts had I been met with detached silence instead of amused encouragement after the first.

The connexion of an entirely casual attitude with success is, of course, well known. I remember once asking Mr G. N. M. Tyrrell, who was convinced he was a hopeless percipient, to take part in an experiment which I was carrying out, with drawings at a distance, for the late Mr Whately Carington. He consented, as a joke, and, to his astonishment, got seven drawings right out of twenty all different. But his later efforts never deviated from chance.

To save correspondence, may I add that I do not quote the above cases as *evidence* of ESP. I naturally do not consider them fraudulent, though others may, and they may of course all be chance coincidences. My point is that, *if genuine*, they are examples, among many, of cases containing features in common

¹ I have seen the envelope said to have been used in this experiment. For what my impression is worth, I should have said the writing was that of an elderly woman.—ED.

which might with advantage be remembered when planning experiments.

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RESISTANCE IN ESP EXPERIMENTS

SIR,—One of the psychological factors in ESP experimental work which does not appear to have received much, if any, attention is what is termed by psychoanalysts Resistance. This occurs when unconscious material attempts to break through into consciousness, and manifests itself in various ways. The patient being analysed may remain silent, since he is completely unable to think of anything to say; he may be evasive, untruthful, forgetful, may make mistakes and slips of the tongue; he may take a dislike to the doctor and even in extreme cases break off treatment.

These and other similar mental mechanisms betray to the trained analyst the anxiety—in ordinary parlance fear—itself largely unconscious, of the irruption into consciousness of emotionally charged material painful to the patient. Similar mechanisms may be detected in normal people approaching the subject of ESP, since there is a general fear of the 'Uncanny', that is, anything which does not appear to obey the ordinary rules of everyday, commonsense life, including such material emanating from deep levels of the personality as cannot be easily assimilated in consciousness. As a lady said to me not long ago when relating a recurrent dream of a house which she eventually recognized and entered in Central Africa, much to her surprise, 'Talking about things like this makes me feel cold down my spine.' In my opinion, such frank confession of shivers actually reveals less fear than the hostility to psychical research displayed by many matter-of-fact people. This fear of the Uncanny, which is biological, has been fully dealt with by Mr G. N. M. Tyrrell in his writings. At the same time it must not be overlooked that there is attraction to as well as repulsion from the Uncanny, and it would be an interesting point to reflect on; what mingling of these factors results in serious, dispassionate psychical research?

My attention was first drawn to this subject of 'resistance' some time ago, when I noticed how frequently reports to the S.P.R. of spontaneous phenomena were delayed, and how often letters in reply to questions asking for further details and confirmation began with apologies for not writing sooner. I began to think that this was more than a coincidence. Later, when I became a member of the S.P.R., I observed in myself an irrational reluct-

ance to report a certain dream which appeared to be both telepathic and precognitive, the circumstances of which were evidential. I rationalized this reluctance by telling myself that the S.P.R. had plenty of material more interesting than mine. It was only some time later that I was able to overcome this inhibition or 'resistance' and report the dream. Freud says: 'It is by no means impossible for the products of unconscious activity to pierce into consciousness, but a certain amount of exertion is needed for this task. When we try to do it in ourselves we become aware of a distinct feeling of repulsion which must be overcome, and when we produce it in a patient we get the most unquestionable signs of resistance to it' (*Collected Papers*, 1925, pp. 26-7).

I noticed when I began experiments in card guessing that I felt an unusual and baffling sense of confusion at first, and a tendency to forget the instructions, simple as they were. This passed off as soon as I became aware that I was resisting. I observed the same air of slight confusion and tendency to make mistakes in my friends taking part in the experiments, although they were all persons of high intelligence and some intellectual attainment. A favourite gambit was to seize a pack of Zener cards as soon as a run of guesses had been made and shuffle it so that the target could not be checked. There was also a tendency to lose interest in the experiments if at the first session no superficially interesting scores could be seen at a glance, and to have difficulty in finding time for further sessions. It may be argued that on my hypothesis the less successful subjects should show less resistance; but it is after all the conscious ego who consents to take part in the experiments, and a little encouragement keeps him at the task, while lack of success discourages him. Moreover, I believe that everyone has psi faculties, and it is probably the strength of his fear which prevents him from scoring significantly; he is like the patient who, finding that nothing spectacular happens at the beginning of his analysis, gives up his treatment, saying, 'There is nothing in psychoanalysis', and is all the time secretly glad of the excuse not to meddle any more with it. The real resistance of the more successful subject sets in later, when he ceases to score significantly and gives chance or negative results. It is as though the psi part of the personality announces that, having given evidence of his existence, he declines to perform any more. Probably the same mechanism works in mediums under investigation, since psi does not appear to wish to give away any secrets.

The analyst has a technique for helping to overcome resistance, which may be summed up in one word, reassurance. This is not so easy as it sounds, however. It depends upon the transference

—the formation of an affective bond between doctor and patient—a situation which needs skilled handling. Psychical research has no comparable technique yet, although a pleasant and friendly atmosphere seems to be so encouraging in helping the emergence of psi faculties in experiments that in my opinion it is essential.

It may be that psychological investigation of 'sensitives', such as mediums, automatic writers, and others subject to marked spontaneous experiences, will throw light on the problem of resistance, and suggest why it should be weaker in some subjects than in others. If, as one suspects, the appearance of psi faculties is connected with a tendency to dissociation, one would hesitate to apply regularly a technique for splitting a normally well-integrated personality for experimental purposes. The work already done with drugs and hypnosis is interesting, but has obvious dangers. As we are all, no doubt, only too well aware, the task of the investigator into ESP problems is not an easy one.

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PSI FUNCTION AND PSYCHODYNAMICS

SIR,—An attempt was made in 1947 to carry forward the study of psychodynamics, recognising 'psi' as an active entity. The nucleus of a research unit was formed. Methods of working were based on techniques and observations made during research in psychotherapeutic fields. Basically this consisted of group and individual work, symbiotically conducted, the dream being used throughout as a projection screen to observe psychic processes. This has applied to work concerned with either spontaneous or statistically assessable phenomena. To date, upwards of 1,200 clinical and research group meetings have been held and over 50,000 dreams carefully examined and many recorded.

Long-distance work has now become a very active branch of the unit. The object of this work is still, primarily, the study of psychodynamics and the basic techniques are the same. Some of the experiments are with Zener cards, and some are of a more spontaneous nature.

Home or overseas members of the Society who would be interested in such work and its approach, and who would care to take part in one or other aspects of the work, are asked if they will kindly get into touch with me.

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G. N. M. TYRRELL

As this issue of the *Journal* goes to press, we learn with deep regret of the death, at Reigate on 29 October, of Mr G. N. M. Tyrrell. Mr Tyrrell became a member of the Society in 1908, and was elected to the Council in 1940. He was President for the years 1945-6. He was the author of *Grades of Significance* (1930), *Science and Psychical Phenomena* (1938), *Apparitions* (the Frederic W. H. Myers Memorial Lecture for 1942), *The Personality of Man* (1946), and *Homo Faber* (1951). An obituary notice will be published later.

INDEX TO VOLUME XXXVI

VOLUME XXXVI of the *Journal*, which is completed with the present number, consists of Nos. 662 to 672 (January-February 1951 to November-December 1952). The Index will be circulated with the issue for January-February 1953.

The cost of the binding case for Volume XXXVI will be announced in due course. In the meantime, readers who require any numbers to complete their sets are advised to order them from the Secretary of the Society, 31 Tavistock Square, London, S.W. 1.



